

and the others as successively less recent in the order in which I have named them, and comparing similar parts of any two zones, the height of the anticlinals is greater, the dip less, and the difference between their axes greater in the more recent." The pamphlet is ably written and very deserving of study.

HENRY H. HOWORTH

### LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

#### Animal Locomotion

IN NATURE, vol. ix. p. 301, there is a letter from Mr. Wallace on a very important point connected with the Theory of Flight. The question he discusses is "whether a bird's wing during onward flight moves *downwards and backwards* or *downwards and forwards*;" and Mr. Wallace supports Mr. Pettigrew in affirming that the movement is *downwards and forwards*.

As this is a subject to which I have paid long and close attention, I desire to express my conviction that neither of the two motions thus described by Mr. Wallace is the true motion of a bird's wing in forward flight.

The true motion is one strictly vertical to the axis of the bird's body; and as that axis is ordinarily horizontal in flight, the wing-stroke is a vertical stroke, that is simply downwards, and neither "downwards and forwards" nor "downwards and backwards."

This is not a question of theory, but a question of fact, to be determined by observation. The wing-stroke of most birds is indeed so rapid that the eye cannot distinctly follow the operation. But there are birds whose wing is so large and whose flight is so slow, that the wing-stroke can be followed with the greatest distinctness. Such is the common heron—common, alas, no longer in most parts of England, but numerous on the west coast of Scotland. When at home I am in the daily habit of watching their flight; and the truly vertical character of the wing-stroke is a fact which I have verified by the eye under every possible condition which could supply the evidence.

There are indeed two slight modifications of the perfect perpendicularity of the stroke which result (1) from the attachment of the wing to the body of the bird, and (2) from the structure of the wing-feathers. The first of these two modifications consists in this—that as the wing moves upon a hinge, its extremity must move downwards, not absolutely vertically, but describing an arc. The segment of a circle, however, through which the wing thus moves, is generally a very short one: and in so far as the movement of the extremity departs from the vertical, it departs therefrom neither "backwards" nor "forwards," but (as it were) "inwards,"—that is, in the direction of a circle encompassing the axis of the bird's body as with a hoop. Pigeons, as an amusement and in play, often complete this circle—making their primary quills clash against each other over their backs, and downwards again under their breasts. But in ordinary forward flight, when birds are intent only on progression, the wings move through a very small arc indeed of the complete circle referred to.

The second modification of the perpendicularity of the stroke arises from the "set" of the wing-feathers—which curve backwards and downwards from the wing-bones. In some birds, and notably in the heron, and all the storks, the concavity thus formed is very deep, and of course a surface which is thus not a plane surface, but a concave one, however truly it may be struck downwards, cannot have a purely vertical reaction on the air.

When we observe, however, that in the case of many birds, and some of these the most powerful fliers in the world, this concavity of the wing-feathers is very slight indeed, and that the whole vane is very narrow, flat, and "taut," it is obvious that a purely vertical stroke, or one as near it as possible, is the really essential stroke for flight.

The great secret of flight is the exquisite and complicated adaptation of structure in the feathers of a bird's wing which derives from this one simple action the resultant of a force which is both sustaining and propelling. It is an adaptation which, when thoroughly grasped and understood, at once dispenses with as needless, and condemns as mechanically erroneous, all the explanations which assume either a "downward and forward" or a "downward and backward" movement.

I venture to think that Mr. Wallace is certainly in error when

he ascribes to Mr. Pettigrew the merit of having been the first to show that "horizontal forward motion is a general resultant of the upward and downward action of the wings under the influence of gravitation."

In February 1865 I published in *Good Words* a paper on the mechanism of flight, in which this effect of the wing-stroke was fully explained, and elaborately illustrated. This paper subsequently appeared as chap. iii. in the "Reign of Law" published in the end of 1866. Mr. Pettigrew's lecture before the Royal Institution (in which I believe his views were first promulgated) was delivered on March 22, 1867. I had the pleasure of hearing that lecture, and the amusement of recognising parts of it (including even a poetical quotation) as taken directly from my chapter on flight. The pleasure, however, was somewhat abated by the strange mixture of much that was quite correct, with a great deal more which I believed then, and believe now, to be wholly erroneous.

ARGYLL

March 11

MR. WALLACE has well said that the question, How a bird's wing moves in flight, "is a very important question." In these days, when scientific attention is being directed to the problem of aerial navigation, it is especially important. I have the less hesitation, therefore, in troubling you with some further remarks in reply to the strictures of this very accurate observer.

At the outset I must deny that I assumed either that a bird's wing is inflexible or that it is a plane. Of its flexibility I had no cause for speaking at all; but so far from regarding it as a plane, I expressly objected to Dr. Pettigrew so representing it in his supposed refutation of the orthodox view. The point in dispute is entirely concerning the down stroke; against Mr. Wallace's account of the up stroke I make no objection.

First, what may we infer *a priori* concerning the down stroke?

(1) Its efficiency is independent of the velocity of the bird: this is simply a consequence of the second law of motion. We have to suppose a bird fixed in still air, and to ascertain the effect which ensues on a downward blow of the wing. The subsequent forward velocity of the bird, so far as that depends on the down stroke, is but a consequence or an accumulation of these effects. It is thus only needful to analyse the single effect itself. To this end the shape and varying flexibility of the wing must be noted. Along the exterior margin we have a rigid area, comparable to the blade of an oar, and formed for the most part of bone, in the *top side* of which the rigid tubes of the primary and secondary feathers are inserted. On the under side of this, which we may term the oar part of the wing, there is thus a considerable concavity, the direction of which when the wing is extended is decidedly backward. The area towards the middle line of the wing is flat and horizontal, approximately so at all events, when the bird is freely suspended in the air. Of the posterior, the larger, half of the wing it is true, as Dr. Pettigrew says, that the aspect is forward, more especially in heavy birds with broad and rounded wings. The flexible extremities of the feathers readily turn upwards like vanes in the manner so well shown in Fig. 80 of Dr. Pettigrew's work. We may thus roughly distinguish four areas, beginning from the front: (a) the oar area; (b) the plane or flapping area; (c) the kite area; (d) the vane area. (2) Now we may inquire what will be the effect of each when the wing is struck downward. The reaction from the oar area will be (a) a force directed upwards and forwards; that from the plane area (b) a force directed upwards simply. Against the kite area will impinge the air sent backwards and rebounding from the blow of the oar area; the effect of this (c) is all that corresponds to what Dr. Pettigrew calls the kite action of the wing. Lastly, the same air in escaping through the feathers, and especially in raising the tips in the vane area, will produce the forward motion (d) to which Mr. Wallace refers, besides contributing something (e) to support the bird's weight. The horizontal component of (a) together with (d) will carry the bird forward. The slighter horizontal component of (c)—slighter because proceeding only from the rebounding air and from a yielding surface—will tend to hinder the forward motion: hence the absence, more or less complete, of this area in quick fliers. The forces (b), greater part of (c), and (e) will sustain the bird against gravity.

Neither Dr. Pettigrew, nor apparently Mr. Wallace, distinguishes the motion consequent on a surface striking against the air from that of a surface gliding through it. If I incline a sheet of paper to the horizon and let it slip from my hand it will descend with a similar incline towards the ground; but if, having stiffened it, I strike it against the air at the same inclination it will tend to rise in a direction at right angles to that inclination. The blow

directed downwards and backwards must give an impetus upwards and forwards; the *still surface* so directed will glide downwards and forwards. I do not deny that if the down stroke of the bird's wing be directed backwards, *beyond a certain angle*, the resultant motion will be, as Mr. Wallace says, "obliquely downwards." But why? Because all the sustaining forces above enumerated are so seriously diminished—the horizontal and forward forces, with the exception of (*d*), being increased—that, to use Mr. Wallace's words, "the surplus vertical reaction of the down stroke over the up stroke is no longer able to overcome gravity," which converts the bird's wings for the nonce into kites as it comes sailing downwards, making but an occasional strike, now that the horizontal effect of the wings is so great, to increase the obliquity of its descent.

But within the limits of this angle, whatever they be, the effect of a downward and backward blow must, on mechanical grounds, be in general such as I have said. For clearness' sake it may not be superfluous to note an ambiguity in the expressions "downwards and backwards," "downwards and forwards;" they may apply either to the direction of the surface of the wing or to the direction of the anterior margin. I maintain only that the direction of the surface—in some wings, merely that of the anterior portion of the surface—is downwards and backwards. The anterior margin, by the contraction of the great pectoral muscle, is drawn downwards and forwards, in which, by the way, there is the further advantage that less air will escape from under the wing in front.

But, secondly, what can we *observe* as to the down stroke?

(1) A fact, pointed out to me by an anatomical friend—that the great pectoral muscle which depresses the wing is inserted into a crest situated on the upper and forward side of the head of the humerus, so as to tilt the under surface of the wing slightly upwards, *i.e.* give it a backward direction. (2) If the flight of rooks, or still better of pigeons, be watched from a window towards sunset, the position of the shadows on the under side of the wings will be found pretty conclusive as to their direction. (3) The forward inclination of the wings of a bird about to alight, which shows that the motion of the wings in such a position retards flight. (4) The action of heavy land or water birds, that have to attain some momentum by the use of both feet and wings before they can rise; here surely a forward blow against the air is manifestly absurd. (5) "The highly-inclined position of a hovering bird," noticed by Mr. Wallace, and not of the bird only, but of his wings.

Mr. Wallace's closing remark is both true and sound:—"A bird's wing is a highly complex apparatus, subject to a variety of flexures and motions in every feather." Still it is possible, even probable, that all this variety is referrible to a few simple principles. It is with these alone that I have ventured to concern myself.

JAMES WARD

Trin. Coll., Camb., March 3

WITH reference to Dr. Pettigrew's letter in NATURE, vol. ix. p. 362, I cannot do better than ask him to read the two papers that I refer to in my former reply, which he has evidently not done.

March 16

A. H. GARROD

### The Moon's Want of Atmosphere

YOUR very suggestive review of Messrs. Nasmyth and Carpenter's work on "The Moon" leads me to propose an explanation of the absence of a lunar atmosphere, which I do not remember to have seen anywhere. The many arguments in favour of the temperature of the lunar surface being near or at the *absolute zero*, when added to the equally probable supposition that at the *absolute zero* all matter assumes the solid form, makes nothing more probable in my mind than that it is the consolidation from cold of all the previously existing gases and vapours of the Moon which has caused its atmosphere to disappear. Prof. Frankland's theory of the frozen condition of the lunar surface is evidently different from the above, and Lord Rosse's observations on lunar radiation apply only to the direct reflection of the solar rays.

A. H. GARROD

### On Volcanic Eruptions

A PASSAGE in Nasmyth's work on the "Moon" suggests, as a consequence, an explanation of volcanic eruptions that I have often given in lectures. The point to be explained is, why they are sudden and intermittent. Processes of cooling and expansion are *gradual*.

I postulate (1) that a solid crust is shrinking as it cools; (2) that the liquid interior expands on solidifying; (3) that the melting-point of lava is lowered by pressure.

Let us start with a volcanic vent in which the aperture has become partially stopped by cooled or solidified lava. In the region below, pressure sets in from the cooling and ultimate solidification of part of the liquid mass. Hence the melting-point of the rest is lowered by (3). The process continues until the pressure becomes sufficient to relieve itself through some vent, old or new; a lava rises in the vent. But this relieves the pressure, and it follows from (3) that more rock will solidify, *suddenly*, and in so doing force liquid rock *rapidly* up the vent.

A volcano is, in fact, a geyser of lava.

I do not remember to have seen this in any book; and it perhaps would have been hazardous to assert postulate (2) as certainly true previous to Nasmyth's experiments; but I have thought it probable: and if it is true, postulate (3) follows, I believe, from the laws of heat, and the explanation will be sound. I shall be glad to hear what is thought of it by authorities.

Rugby, March 13

J. M. WILSON

### Remarks on Ozone

HAVING perused Dr. Moffatt's interesting communication on Ozone in the *Scottish Meteorological Journal* of October last, and also noticed the paragraph on the subject in the *Medical Times* drawing attention to it, I beg to send the following remarks respecting some points in it open to criticism from outsiders:—

1. The numbers, and special *years of records*, are not stated in the statistical tables, which might be of importance for comparison with other persons' records.

2. The occurrence of ozone with *hail*, and not with snow, may be explained by its happening in warmer weather, and not in winter, and in the warmer stratum of air through which the hail falls from the cold stratum above.

3. The larger quantity of ozone in Table II. in *winter over that in summer* is anomalous, and inconsistent apparently with the records in Tables VI. and VII., where it is stated to increase with the temperature.

4. If ozone be thought to increase in quantity with *increase of elevation* above the level of the sea, it may be asked how that is to be reconciled with the greater prevalence of it at the sea-shore than inland.

5. If there be only an apparent connection between *electrical storms* and ozone, explanation may be required to account for the production of artificial ozone by electrical action, and whether the two be identical in constitution if not in origin.

6. The paragraph—"The air is *drier* near the tropics than about the equator," might be more clearly defined by adding the "tropical circles of cancer and capricorn," as within those lines it certainly gradually gets more and more humid.

7. In one paragraph there is stated to be an intimate connection between *humidity* of the atmosphere and the manifestation of ozone, and in another this is stated to be purely accidental, which is ambiguous, while the testimony in support of it is not in accord with that in Tables IX. and X., where the adverse record is apparent.

8. That the absolute *humidity of the air* diminishes with increase of elevation may be true in the case of lofty balloon ascents, away from any terrestrial influences of mountains, but, as pointed out in a note, the *relative humidity* increases, as we may see in Westmoreland or Dartmoor, where the heights are always misty and damp.

9. The tropical or *trade winds* only chance to be land winds in some such regions as the North Indian Ocean, whereas they are generally said to be sea-breezes, as in the South Atlantic Ocean, in the ordinary acceptance of the term.

10. The connection of the production of ozone by the means of *turpentine* will bring to the mind of the tourist the freshness of the air of hills planted with pine forests.

11. Accepting the theory that ozone is connected with the equatorial winds, it may be asked how the increase of ozone in the *calm belts* is to be accounted for, where there are only Polar winds, converging to ascend into the upper regions of the air from north and south.

12. The table of *observations at sea* on board ship would require to be supplemented by a note of the period of the year and number of days on record, as the quantity of ozone is already stated to vary with the *seasons of the year* (Table II.), and